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COMMENT ON PROFESSOR BENNETT'S REPLY.

In accepting the opportunity which the editor affords me of adding a few words of comment to Professor Bennett's *Reply*, I fear that I shall only add to the appearance of futility which such debates often afford. Certainly, the disputants do not convince each other—a disability which seems to be inherent in the philological nature—and it is at least doubtful whether enough spectators will sit through to the still retreating end to make their persuasion worth while. But if there be those still interested in the question, I beg their indulgence for a few minutes while I endeavor to take account of stock and to see what is left of the cause of rhythmic accent.

In the first place, then, Professor Bennett does not defend against my criticisms the meagre evidence which he adduced from ancient sources in support of his own view; nor, indeed, in the form in which it was there presented does it admit of defence. At least so much would seem to have been accomplished.

His theory, then, still remains an assumption not resting upon evidence—'arbitrary,' I called it, perhaps wrongly—or better let me say, an hypothetical assertion, viz. that *ictus* or *thesis* is quantitative prominence. Now, the fact that he reaffirms this position and adds an elaborate rebuttal of positive evidence, collected with a view to proving that *ictus* or *thesis* was a rhythmic accent of variable intensity, adds at best only negative strength to his position.

Turning to Professor Bennett's criticisms as set forth above (p. 414), I had endeavored to show that his primary position was susceptible of theoretical refutation, in that Aristoxenus recognized the possibility of a rhythmic series of primary times (pyrrichs), i. e. without quantitative prominence, but criticised such a rhythm as yielding too rapid a tempo. Professor Bennett has advanced nothing to show that my inference from the words of Aristoxenus was not correct, but he seeks to invalidate the significance of evidence based upon a rhythmic form not in use. He lays special stress, however, on the fact that Aristoxenus' reason for rejecting the pyrrhic series was probably a wrong one, and that the true reason is that such a rhythm does not present quantitative variation—obviously only another way of saying that it can not be characterized by quantitative prominence. But that is to admit that the words of Aristoxenus, whatever their intrinsic value, do refute his theory, and only so much did I claim. As to the correctness of Aristoxenus' observation, it should be remembered that a pyrrhic series need not have been an abstract notion of 'a

non-existent entity,' any more than $\frac{2}{3}$ time in modern music, an exactly analogous case, of which there are found examples, though very rare. In another connection Professor Bennett commits the same logical blunder of declaring my inference wrong, where, in fact, he calls in question the correctness of the observation or method of the ancient source. I had concluded from Gell. VI 7 that Annianus (following Probus) invoked the aid of rhythmic accent to determine certain word-accents in Plautus and Terence. Professor Bennett in reply shows how inadequate the method is (as I had already implied) and declares it 'arbitrary and fantastic.' Had he been content with this I should have nothing to say, except to add that, nevertheless, Annianus and Gellius made use of it; but on what ground does he impugn the justice of my inference¹? In these cases it may fairly be urged that Professor Bennett's method is at fault, in that he has aimed to prove too much—not only that my conclusions from ancient evidence are wrong, but that the observations and method of the sources themselves are erroneous.

In the following I shall select only one point for the reaffirmation of my positive evidence, which I think touches most closely the weakness of Professor Bennett's position. I shall take as my starting-point Aristoxenus' statement that the same rhythmizomenon may yield different rhythmic forms, i. e. the rhythmizomenon may remain constant, the rhythm may change. But Professor Bennett's contention is, that quantity alone yields the rhythm. Here we have again a clear logical conflict between Aristoxenus and the theory of Professor Bennett. His method of escaping the difficulty is to assume that in feeling or consciousness such ambiguous rhythmic possibilities are conformed to the dominant mode. This is *per se* not unreasonable nor impossible. Accordingly, therefore, a consciousness of prevailing quantitative prominence plays in ambiguous rhythmic groups the rôle which is usually assigned to rhythmic accent. Now, it is conceivable that such an explanation would suffice for the quiet reading of ancient poetry to oneself; but is this all that is demanded? When Aristoxenus says that the foot is the means of making known the rhythm to the perception, does he mean the reader only and not also the listener? Surely not; and it must have been the duty of the singer or declaimer, who already possessed a correct feeling for the rhythm, to make it clear to the listener, so that, e. g., the opening of the Pythian Ode referred to above (p. 415) should be felt as a trochaic series (Rossbach) and not as a dochmiac (Boeckh), or such an initial verse as *fundite fletus, edite planctus* should be recognized as anapaestic, and not dactylic. Granted that the reader, from the perusal of the whole poem, might feel the rhythm aright, is there any means by which he could convey

¹ Or does Professor Bennett think that the verses are only cited as containing examples of the words in question and not to show how the ancients pronounced them? Cf. Schoell, *De accentu ling. Lat.*, p. 26, n. 1.

the correct feeling to another in the case of a succession of short syllables, or actually against the quantity (as in the last illustration), except by something superimposed upon the quantity? So much for considerations of a theoretical kind. The most convincing evidence on this point for Latin verse, so far as I am aware, is to be found in the passage which I adduced from Caesius Bassus giving directions for the treatment of dactylic feet in iambic verse, on which Professor Bennett comments above (p. 424). I think I may trust the majority of the readers of my earlier article to believe that I entertained no doubt about the significance or value of the passage in question, although Professor Bennett seems to have so understood the language which I there used. Now we have seen that even though it were possible to preserve a correct feeling for a rhythm which the quantity does not reveal, this consciousness is incapable of interpreting the correct rhythmic feeling to another. Modern music in such cases marks the difference in rhythmic effect by the only conceivable means—a shifting of the bar line or accent; and what says Caesius Bassus? "That iambic verse will seem to lose its character when it admits dactyls, unless you so handle the rhythm,¹ by means of the *percussio*, that when you mark the time (*pedem supplodis*), you strike the foot (dactyl) as an iambus." And then, after the example *exclussit revocat*, he continues: "Strike this on the first syllable and you will apparently have an hexameter." I suppose it would be quibbling to urge that Caesius says you must modulate the foot in such case by a stroke or a blow, and not by a consciousness of prevailing quantity; but, frankly, is it reasonable to believe that all these words, which have so suspicious a resemblance to notions of stress and intensity, are a way of describing the necessity of maintaining a subjective consciousness of the iambic character of the rhythm?²

¹ *Moderaveris* is probably here a term. tech. = ῥυθμίζειν (*modus* = ῥυθμός), but I have no axe to grind by this suggestion.

² The passage of Caes. Bassus I am content, as before, to leave to the judgment of the reader. But Professor Bennett has treated the passage which I cited from Servius, and my remarks upon it, so strangely that I feel it worth while to go into his criticisms in some detail. I had pointed out that Pompeius and Julianus, by the introduction of trisyllabic words as illustrations, had completely distorted Servius' remarks on the distribution of trisyllabic feet between arsis and thesis. I pointed out that this was obvious, if one observed the chronological sequence—Servius, Pompeius, Julianus. Professor Bennett holds to his original view, that all are dealing with phenomena of individual words and not feet, and affirms that the two later grammarians are not dependent on Servius, but draw from a common source, Donatus. But the *Ars* of Donatus is not a lost book, and the matter is of easy verification. If the reader will refer to Donatus *de pedibus* (IV 369, 18), he will see that the matter is not to be found there, but is an amplifying comment introduced apparently first by Servius from some such source as Marius Vict. (VI 49, 22), as I intimated before. Furthermore, if the reader will refer to Pompeius ad loc. (V 120, 30 ff.) he will observe that the indebtedness to Servius is so notorious that Keil has set against the text the corresponding pages of Servius. If, then, he will look up one of the references which Professor Bennett gives

Professor Bennett concedes (p. 418 above) that muscular movements of alternating contraction and relaxation might attend naturally regular alternations of long and short quantity. He does not think that they indicate a mental sensation of intensity nor corresponding vocal stress. But why discriminate against the muscles of the respiratory organs and the voice? If, in response to quantitative variations, the foot stamps, the fingers are snapped, the long syllable is struck (*percussio, ictus, ferire, caedere*, etc.), it would seem to have required some conscious effort of control if the voice should not participate in such feelings. And what else than such participation led the grammarians to reverse the Greek terms and apply them to the voice, designating the prominent part of the foot as *elatio (elevatio) soni, vocis*, the less prominent part as *vocis remissio, depositio*?

I pass to a brief review of some of Professor Bennett's criticisms of my arguments. He repudiates the application of results of psycho-physical experiment to this question, as having been made upon Teutonic subjects—and with some measure of justice. But the most essential result, the association of rhythmic feeling with muscular movement, so that any objective rhythmic phenomenon is interpreted by corresponding muscular contraction and relaxation, is inherent in the constitution of the human body. Nor can any one who has watched popular Greek and Italian dances doubt that the association of stress with duration in rhythm is as natural to the descendants of the Greeks and Romans as to us Northern peoples.

In regard to the verse of Plautus and Terence, Professor Bennett assumes (p. 425) that I have dipped into wide, controversial questions and selected the special points of view that make for my argument. Would it not have been as fair to believe that because of my attitude toward the questions involved I was led to reject his theory of rhythm? But I see no reason why we need be involved in all the unsettled questions of early Latin verse. In fact, on the essential point that I raised there is no *general* controversy, viz. that long syllables are shortened under the influence of an adjacent verse-accent. The controversy is concerned with the limits of the application of this principle. Klotz, who has carried it furthest, has met with much criticism in detail, but in all the reviews of his work that I have seen I have found none which repudiates the principle. When Professor Bennett (p. 419 above) seeks to implicate me in other views of Klotz to which I

(G. L. V, praef. 91), he will read the definite statement of the editor that Pompeius seems to have used Servius 'pro fundamento disputationis suae.' Finally, Professor Bennett says that his view is confirmed by the fact that the grammarian Sergius represents the same confusion. If reference were not made to Keil, I should be constrained to think that there was some essential divergence of text here, since neither in IV 483, 14 (an error of citation, apparently), nor 480, 14 (where *arsis* and *thesis* are touched upon), does Sergius cite examples like those of Pompeius and Julianus, nor does he discuss the division of trisyllabic feet.

did not appeal and which I have never defended, he deserts the field of discussion.

In regard to the passage of the Annals of Ennius which I cited, I was certainly under a misapprehension as to the real proportion of dactyls and spondees in the verse of Ennius, and Professor Bennett makes it clear that my chance illustration is not representative.¹

I have held and I still hold that the question is primarily one of evidence and the interpretation of evidence, and not a matter to be determined by considerations of an *à priori* character. But since Professor Bennett is of opinion that the refutation of his theory demands a consideration of four other theses besides the one relating to ancient evidence, I will touch upon these briefly; although, had the positive evidence that I sought to advance carried conviction, I should certainly not bother myself about 'the irreconcilable contradictions' which the other postulates present. (1) The first thesis declares that Latin was a quantitative language of level stress, and certainly my case labors *in extremis* if it is demanded that I should controvert this statement. If by level stress Professor Bennett meant absolute lack of dominant accent, there would be some *point d'appui* for discussion. But, in fact, if I were to set forth my own views in regard to the practical reading of Latin verse, I should wish to make use of the acknowledged principle of 'level stress' (Seelmann, *Aussprache* etc., p. 372, 1). Obviously, then, this is not a thesis peculiar to Professor Bennett's position, but a generally accepted description of the vocal character of the Latin language. (2) The second thesis declares that quantity was the basis of Latin rhythm, to which I must also give my assent, only adding my belief that recurrent quantity carried in its train a rhythmic accent of variable intensity, smoothing out the inevitable unevenness of quantity pure and simple, and moulding the rhythm. (3) The third thesis is the first one that calls for criticism, or refutation. This affirms, that if the psychological element which characterizes rhythm is a time-element, a second principle of rhythmic grouping (stress) could not exist simultaneously with it. The only reason assigned for this statement, so far as I have observed, is that 'a second principle being superfluous, seemed impossible'—as one would fain say of the vermiform appendix. The most obvious refutation of this thesis is to be found in the illustration which modern music affords, of a perfectly developed quantitative system of rhythmic grouping, accompanied by another rhythm-producing element, accent, as indicated by the bar line which is the sign of accent. One might go further and point out that, aside from these two

¹On *contractio* (*syllabarum*) in the sense of 'shortening' (above, p. 424), see any dictionary, s. vv. *contractio*, *contrahere*. With reference to Professor Bennett's concluding words, I should not have suggested that his statement was dogmatic, had I realized that 'ancients' referred to the Roman grammarians.

primary rhythmic principles, there are in music other subsidiary means of producing rhythmic effect, such as pitch, *timbre*, melodic phrase, etc. Indeed, so far from its being true that one form of rhythmic grouping excludes another, it is one of the most certain results of rhythmic study that different means of producing rhythmic effect tend to combine and to reinforce one another. (4) The fourth thesis refers to the 'stupendous artificiality' which the assumption of rhythmic accent introduces into the reading of Latin verse. But the artificiality and its magnitude must be due to some other reason than because Latin verse is quantitative. For, as I have just pointed out, one form of rhythmic expression attends another as its natural concomitant, and rhythmic accent is produced unconsciously and naturally as the result of quantitative variation.

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